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A Tale of Action-Research in Information Systems: Memories of a Ph.D. Student

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A TALE OF ACTION-RESEARCH IN INFORMATION SYSTEMS: MEMORIES OF A PH.D. STUDENT

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ABSTRACT

This paper tells the tale of a Ph.D. student who used action-research to establish an information systems design methodology. It describes how he initiated his project, how he came across action-research, and how he slowly evolved in his search for coherence until he felt he could progress confidently. The paper is mainly intended to offer to readers who are unfamiliar with action-research a context where they can become aware of some of the difficulties of its use how they can be solved, and recognize some criteria that can be met to ensure that the results obtained are scientifically valid. The paper concludes by reflecting on the close relationship between action-research and the epistemology of the information systems field, suggesting that this relationship should be the object of further exploration.

Keywords: action-research, rigor, relevance, validity, information systems design

I. INTRODUCTION

This paper is a story, the true story, of a Ph.D. student who used action-research to develop a new methodology for information systems design. Story-telling has been, since the most primitive oral cultures, a very powerful way of inducing knowledge, facilitating memorization, conveying meaning, and creating sense. In the 1930s, the University of Colorado physicist Georges Gamow demonstrated, with the publication of his much acclaimed Mr. Tompkins in Wonderland [Gamow, 1939], that writing a story, even when it is a long one, can be an extremely effective way of conveying to adult readers such complex concepts as relativity theory. Through Mr. Tompkins' eyes, as he traveled to imaginary worlds, the readers became familiar with many central concepts of modern physics, from atomic structure to relativity, and quantum theory to fusion and fission. Captivated by those stories, Tom DeMarco borrowed Gamow's character in his book The Deadline [DeMarco, 1997], a witty novel about the principles of software project management. Kieran Egan, a professor of education at Simon Fraser University, explored at length the unique virtues of story-telling in human learning [Egan, 1997], and Birchard, among other recent authors, emphasized the strengths of story-telling as a valuable device in the toolkit of a manager [Birchard, 2002]. Even more recently, Steve Denning wrote in detail about the use of stories to motivate people and share knowledge in organizations [Denning, 2004], [Denning, 2005]. Writing, in general, and story writing in particular, are also being recognized as particularly relevant ways

of carrying out qualitative research [Richardson, 1994], and Eden and Huxham [1997] suggest that it can be a powerful way to disseminate the results of action-research.

Encouraged by these reasons, we have decided to describe in the form of a tale the true experience of one of the authors when he was a Ph.D. student using action-research to establish an information systems design methodology. Although the paper has two authors – the Ph.D. student of the tale and his advisor – it will be told from the perspective of a single character: the Ph.D. student.

The research described in the tale took place over a period of approximately four years. It begins with a description of the final months of the student's M.Sc., when he started realizing that existing information systems design methodologies were becoming obsolete, and this suggested that his Ph.D. thesis could be devoted to the investigation of alternatives. It then illustrates how, in that process, he discovered action-research, became enthusiastic about it, and experienced the first difficulties of the newcomer. It moves on to his reflections about the generalization of the results of action-research and to the extension of his work after the thesis was finished. The paper concludes with a final summarization and reflections on professional issues. In Appendix, a brief description of the methodology resulting from this action-research process is given.

II. METHODOLOGIES THAT DIDN'T DELIVER

Late 1996. My beliefs about information systems design started to come apart in the final months of my M.Sc. I had been carrying out the strategic analysis of my university department, along the lines suggested by the Positioning School [Mintzberg et al., 1998], and was about to draft the "blueprint" for its information system based on my findings. This was when my problems started cropping up. There I was, before some full-blown legacy applications that the department wanted to keep, some high-level requirements for custom coding the specific modules that would give us strategic advantage over other universities, plans to buy a couple of commercial packages, and a massive intranet component to tie all this up together. How could I accurately describe intranet pages that were 90% comprised of information collected on-the-fly, from a handful of underlying old legacy applications and home-made databases? How could I integrate all that with several new, common off-the-shelf (COTS), black box packages that were never meant to talk to each other? What about email messages and mailing lists? How could I represent these in my information system blueprint? Where did the elegant methodologies I knew about – Structured Systems Analysis and Design Method (SSADM) [CCTA, 1990; Eva, 1994], IBM's Business Systems Planning (BSP) [IBM, 1984], Martin's Information Engineering [Martin, 1990] or Merise [Quang and Chartier-Kastler, 1991] – fit in? Trying to represent the information system forming in my mind using their diagrams and concepts was like trying to fit a square peg into a round hole. Function definitions? Logical data models? Physical process specifications? Relational data analysis? Processes vs. data type matrices? Data entities? Cardinalities? How could that match the dynamic intranet pages I was supposed to define and the ready-made packages that were to be purchased?

My advisor seemed to be mysteriously pleased with my discomfort. He had been mumbling something, for months, about the limitations of the traditional approaches, but I had been too busy to worry seriously about that. He seemed to be attracted to what he had described as object-oriented approaches to enterprise information system design. So as he suggested, I tried to get some inspiration from a book he had just come across, "Getting Results with the Object-Oriented Enterprise Model" by Gale, Eldred and Odell [Gale et al., 1996]. Since I was in trouble, I decided to devote a whole weekend to the book, but I came out of it even more unconvinced than before, while incurring a two day delay in my schedule. If this was an object-oriented model for information systems design, then I didn't want to have anything to do it! It was just more stuff about elegant architectures, with no answers on how to account for complete legacy applications, web services, and the like.

Weren't there any clues, anywhere, on how to describe this ordinary information system? Or was it not so ordinary after all? Since I got no answers from the bibliography, I had no choice but to do my own thing. So I used quite a lot of improvisation and mixed ideas from strategic analysis and software engineering to describe the whole lot the best I could. I wasn't very proud of the result, so I prepared to be beaten up during the public discussion of the master thesis.

July 1997. Well, that was not so bad after all. The master degree is in the bag! If the jury had better insights to my problem, it sure didn't share them. And there were some quite fussy people on that bench, too. Maybe this was a good subject for a Ph.D.. I would need to check with my advisor to determine if he would put up with me again, this time for the next three or so years. After I enjoyed a full month of well deserved summer vacation, of course!

III. STARTING A PH.D.

It was settled. My advisor was glad that I was finally paying attention to the inadequacy of traditional approaches, so he promptly agreed to supervise my doctoral work. And it seemed that he was prepared to give me quite a lot of leeway. When I persisted that I wanted to steer clear of Gale's object-oriented approach [Gale et al., 1996], he didn't insist. Although I got the feeling that, deep in his soul, he thought that I was just begin stubborn and would need to hit a brick wall before I would come to my senses. One thing we both agreed on was the need for me to go into the field. So, we decided to leverage the relationship that our research group already maintained with a technology transfer institute where private companies and public institutions frequently received help with their information systems projects. We believed that the immersion in real cases would clarify my unnerving feeling that something was wrong with traditional information systems design. This feeling was, after all, the closest thing I had to a research question! This reminds me… I should write it down…

My research log book:

- _Today I've officially began my Ph.D._

- _Something is wrong with traditional information systems design methodologies!_

- _Tried and failed to represent my department's information system using various well known ones._

- _There seems to be a mismatch between the concepts they use and the reality I was facing._

- _Is this an isolated case?_

The first project assignment I received from the technology transfer institute was to coordinate the development of an information kiosk for one of the top pulp & paper manufacturers in Europe. They wanted the kiosks (which would be distributed throughout the plant) to continuously show up-to-date production figures, as well as other business data. When I examined the "core" of the company's information systems, I felt that I was probably barking up the wrong tree as far as my Ph.D. was concerned, since the application I was looking at, custom developed in a 4th Generation Language several years ago, seemed to fit nicely with the methodologies I was so unhappy about. A more careful look, however, revealed that this 4GL system and its huge database was only part of the picture, since several other applications, namely well known commercial packages, were also at play. The information system was not a huge homogeneous entity, after all. Besides, if this company ever decided to deploy an intranet, like the one I designed for my department at the university, they would face problems similar to those I had to deal with in my M.Sc. All things considered, it didn't look so different from the case of my department.
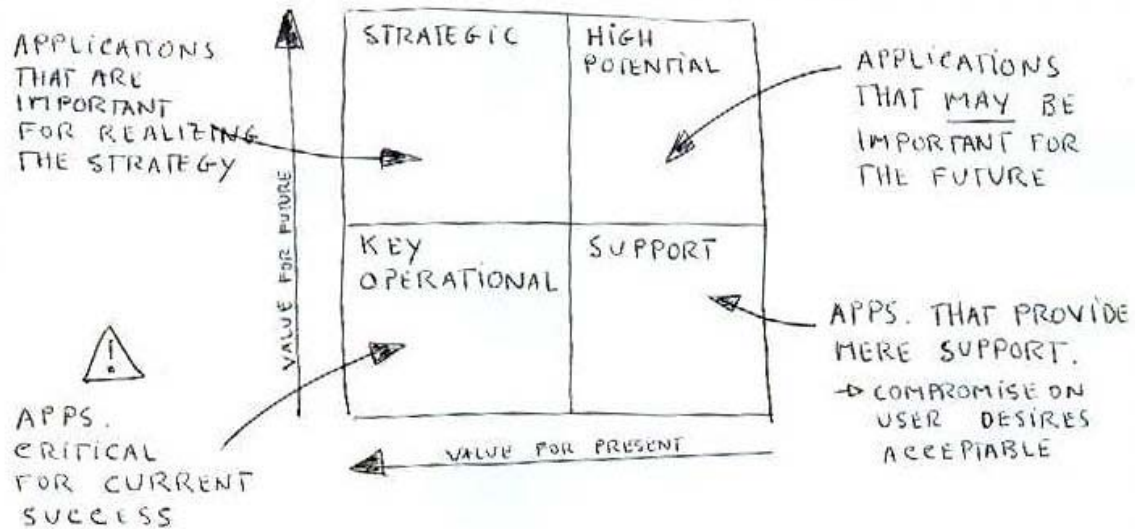
My research log book:

- *The kiosk project is completed. Although it narrowed down to a simple software engineering effort, it gave me the chance to delve into the whole information system of the company.*

- *Most of the data I needed resided in a custom developed application, but there were several other autonomous packages interconnected with it. The overall information system was a mosaic of heterogeneous systems. Seemed a bit messy!*

- *Come to think of it, "my" kiosk became a part of that information system. So, I've added up to that mess!*

- *There was no intranet. If that information system ever evolves to include one, it will be hard to draw a diagram that clearly shows how the data from all those underlying applications will make up the web pages.*

    …at least with the methodologies I tried in my M.Sc. I'll keep looking for solutions.

My second project was not to be taken lightly. The Ministry of Justice wanted to introduce wide computer support in law administration, so several major international consulting companies and software vendors were invited to present their views and solutions. To provide some common ground, the Ministry supplied a typical collection of requirements from one major courthouse, taken as a pilot case. My job was to attend the presentations where the companies made their sales pitch, ask them difficult questions, and then write up an assessment report. This all seemed very nice, but, from the start, there was something troubling me about the pilot case, although I couldn't put my finger on it. Granted, the requirements were no more than a compilation of "wants" and "needs", probably resulting from a call for contributions inside the courthouse, not an in- depth study made by information systems specialists. But, there was something else about it that made me uneasy. The presentations didn't help a bit. Each vendor or consultant decided to go his/her own way, putting emphasis on such dissimilar issues as the generic product that would do wonders for office automation, the system that they had been using for law in countries with completely different judicial systems, the wonderful custom-made application that they would build in a handful of months, or the most imaginative development "methodology" to approach the situation. What a chaos!

It was time to write my report. I was in serious trouble. I had no idea on how to start, let alone how to structure the document. Somehow, the case of the pulp & paper manufacturer also kept coming back to my mind. It was during the weekend that I suddenly had an insight! Of course! The problem with the requirements provided by the Ministry was that they were all mixed up. Some issues would allow a quantum leap in justice, others were merely helpful, and some of them were just promising. They were so different, that there was no way that one custom-developed monolithic application could effectively take care of them all. It was just like the pulp & paper manufacturer case, where the information system was, in fact, a collection of autonomous components, with different values for the business, treated differently, according to their importance. This perspective would allow me to evaluate the various presentations according to the relevance of the system they focused on. Now, all I had to do was to come up with a classification that could be understood by the decision makers to whom my report was targeted. I mentally reviewed my favorite strategic analysis instruments and sketched a McFarlan grid [McFarlan, 1984; Edwards, et al., 1995; Ward and Griffiths, 1996].

APPLICATIONS THAT ARE IMPORTANT FOR REALIZING THE STRATEGY

STRATEGIC   HIGH POTENTIAL

APPLICATIONS THAT MAY BE IMPORTANT FOR THE FUTURE

VALUE FOR FUTURE

KEY OPERATIONAL   SUPPORT

APPS. THAT PROVIDE MERE SUPPORT.
→ COMPROMISE ON USER DESIRES ACCEPTABLE

⚠

APPS. CRITICAL FOR CURRENT SUCCESS

VALUE FOR PRESENT

It seemed to fit the role nicely.

My research log book:

- *When we consider designing an information system for the entire organization we are met with "needs" from its various divisions and processes.*

- *To be manageable, this holistic view over the whole information system "needs" must use some sort of classification mechanism. But established requirements prioritization from software engineering – such as the MoSCoW approach (Must have, Should have, Could have, Won't have) –, won't do, since, coming from different parts of the organization, the "needs" won't be easily comparable. We need a simple and effective way to correlate each potential information system module with its benefits for the organization as a whole.*

- *McFarlan's Strategic Grid came through, in the Ministry of Justice project. I guess I'll stick with it, for now.*

The phone rang. It was a close acquaintance, the Chief Information Officer of the national plant of a large European automotive manufacturer. He wanted a piece of advice regarding some network configurations. Once in a while he used to call me, so I got to know a bit about their operation and information system. But it wasn't until now that I had started to relate that knowledge to my study. The pattern seemed to repeat itself: these guys had a myriad of autonomous systems that were made to work together, if sometimes at painstaking cost! My favorite was the old mainframe in charge of controlling all plant-floor production stages and parts. They also seemed very proud of it, "Do you know how many years this keeps going without a reboot?". It was true. Reliability was of essence. You can't just stop the plant because the computer hung up. The option for that particular system was a highly planned and formal one, made several years ago. Also, if there were any signs of even the slightest hiccup with it, a specialist would be flown in. My mind kept wandering through their setup as I continued the conversation. Other systems are not so critical, but even in those cases a very cogent argument had to be made before replacing any of them. "These things cost a lot of money", they once told me. "Besides, people already know how to

operate the applications. Do you have an idea of how much it costs to retrain staff? Not to mention the usual resistance to change you can get from users. That can really send your beautiful plans down the drain". I hang up. Although I had officially declared this as a rest day, I couldn't resist adding an entry to my research log book:

My research log book:

- *Systems don't have to be state-of-the-art; they just have to do their job properly. If fact, sometimes it is best to keep away from the most recent technology, especially if our main concern is reliability!*

- *Replacing a working system is serious business. We must account for potential resistance to change, the cost of retraining users, and decreased productivity in the first few months, and, of course, the cost of the new system itself.*

- *Keep this in mind: an information systems design methodology must embrace legacy, not reject it!*

These seemed good, solid points, but I kept staring at the log book. Something was still missing. Then I wrote:

- *The importance of classifying the parts of an information system according to their business value goes beyond deciding which ones should be implemented first. It also has to do with how these systems are maintained, once installed. Some of them are so critical to the business that they need 24x7 attention and immediate intervention to bring them back on-line in case of failure. Other, less important, systems don't require this level of service.*

It was now the time to lay back and reflect on all those rich experiences, trying to reconcile lessons learned with old knowledge and the most recent readings. But peace of mind couldn't last long. My advisor was just coming back from the Ph.D. discussion of a fellow student who had been working on the information system for a big shipyard. Although, it seemed, he did a wonderful design job, which the company acclaimed, one of the members of his Ph.D. jury kept asking how he could prove that his solution was the best, or even a valid one, from a scientific point of view, of course. How could he claim that he didn't just get "lucky" in making the design as he did? This time, my advisor looked genuinely concerned. Although his student had passed the examination, he swore that neither he nor any of his students would ever be caught off guard like that again. And so began our deep interest in the philosophical foundations of information systems research.

My work proved to be one of our first challenges in this respect. What I needed to do seemed contrary to all that was established in mainstream research approaches. There was no way I could even aspire to know all relevant variables involved, let alone control them on an individual basis. No two settings – in my case, organizations – would be exactly the same, so no chance of having control groups. Any change made to a setting would permanently affect it. There's no resetting people, that's for sure. So, there was no way I could trace back to the initial conditions to try different alternatives. For that same reason, it would be impossible for other researchers to reproduce my "experiences" to confirm my findings. That's what you get for researching into situations where you have to deal with human beings. How was I going to do that in a rigorous way?

My research log book:

- *I'm feeling depressed!*

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IV. DISCOVERING ACTION-RESEARCH

I was just getting ready to head home when my advisor called me from his office. He wanted to tell me about something mysterious called action-research. He had found a very interesting document that, not only, described this research approach, but also discussed the concerns of using it to produce a thesis. It also contained annotated references to learn more [Dick, 1992]. It looked very promising, so I printed a copy and started reading it very carefully that same night.

I was very excited. After two intense weeks devoted to learning about action research, it all made a lot of sense in the context of my work. Then the dark clouds arrived. Peter Checkland's Soft Systems Methodology (SSM) [Checkland and Scholes, 1990] seemed to mean trouble as I went deeper into its study. Although the description of the methodology in the paper originally found by my advisor was very inspiring, as was the underlying philosophy of the books I read [Checkland, 1981; Checkland and Holwell, 1998; Checkland and Scholes, 1990], some of the nitty-gritty field rules presented by Checkland in some parts of his texts seemed incredibly bureaucratic and, at times, even contradictory. Profound doubts and confusion, caused by the assertiveness of an author with a thirty year track record on the subject, invaded my mind for quite some time. Only after several helpful discussions with the other members of my group, and by mail with Bob Dick, I finally managed to move on. By reading additional bibliography [Dick, 1997; Dick, 1999a; Dick, 1999b; Dick and Swepson, 1994; Jick, 1979; Rapoport, 1970; Susman and Evered, 1978] I was eventually able to sort out the occasional disagreements and establish a solid set of guidelines for my field work. In the end, Soft Systems Methodology managed to have a strong influence, but in the "near mode 2", more "fluid", form. I was finally confident that, of all the approaches that we had been considering, action-research was the one most suitable for the job. More surprising, though, was the fact that I seemed to have been doing things right from the start, in spite of no prior knowledge about action-research. Much later I realized that this had to do with my original training as an engineer. In using their "theoretical" knowledge to solve real-world problems, engineers usually engage in a conversation with the particulars of the case before they are eventually led to the final solution. Then, they look back on how the solution worked out, to get added insights for future projects. These two stages are described by Donald Schön as "reflection-in-action" and "reflection-on-action" [Schön, 1984]. This is the spirit of action-research. I could now re-read my research notes with a whole new light, in a more systematic fashion, and progress with a refined research process. It was also time to create a new section in my log book:

My research log book:

   PHYLOSOPHICAL ISSUES

- *Action-research seems a promising approach for my case. It is used to act on an existing problematic situation with the dual aim of improving it and expanding the knowledge on the subject. Traditional researcher independence is, thus, neither required nor assumed. Participation of research subjects is also possible.*

- *It possesses a cyclic nature – an intervention is planned, the corresponding action is taken, inducing change that, hopefully, leads to an improved situation and, finally, a critical reflection on the results is carried out. Adjustments can be made before the next cycle.*

- *Action-research can, thus, depart from a fuzzy situation and converge to a clear solution as knowledge is built up from one cycle to the next. This contrasts with the need to start with clearly stated research questions, typical of traditional research approaches.*

- *I couldn't help noticing the striking closeness with the practice of engineering! Must remember to explore this avenue in the future.*

I hurried to the meeting. According to my advisor, the head of a University office devoted to the graduate-employer interface wanted our help with their information system. As we listened to his story, we soon realized that the big success of the office was pushing their current system – built by the local hobbyist – to its limits. Since it was a dead end, we all agreed to start afresh and create a brand new one – a rare luxury. For added flexibility, the system should be used through a web browser. All we needed for this philanthropic project was manpower. Later that day we discussed the project with a visiting professor. He had been a consultant for over twenty-five years, part of them at the service of IBM. Being a former Big Blue guy, he had a natural inclination towards Business Systems Planning (BSP) [IBM, 1984], which he taught in combination with other approaches presented by my advisor. When he suggested that we should let a group of his 4th year students approach the problem using BSP, we all felt it was a good idea. I, for one, was very curious to see if, with the help of a veteran, they could overcome the problems I had started to diagnose on traditional methodologies. A few months later the students had completed the planning of the office information system according to BSP, and presented us with the Create, Read, Update, Delete (CRUD) matrices. It was then that the problems started to emerge. Translating those matrices into an accurate description of a web based information system revealed a huge gap: they provided no help on how to structure the user interface in terms of data and its manipulation. To my agenda, this project contributed with the identification of concrete issues where traditional methodologies were showing limitations, and with the confirmation, under new circumstances, of conclusions from previous cases. My "research question" was starting to converge into a more concrete statement!

My research log book:

- *There is a gap between the deliverables of traditional methodologies and the reality of today. Matrices, data diagrams and function definitions can't handle modern intranets and their linkages to a mosaic of underlying applications.*

- *Maybe prototypes, even non-functional ones, can help in the description of their highly dynamic web pages.*

- *Maybe my research question should be: "Can I conceive a methodology that overcomes the gap of traditional methodologies?"*

It was a memorable day when I came across a new paper. Finally, well known authors in the field of information systems design were stating that traditional methodologies were becoming inadequate [Avison and Fitzgerald, 1999]. It wasn't just my imagination, after all. It wasn't just some obscure bias that was influencing me. After refining my search, I found some more texts along the same line of thought [Baskerville et al., 1995]. These dated back to the period of my master's when I started feeling the problems that eventually led me to where I was now. I felt reassured. I was on the right track, and action-research had led me there from an initially vague state of affairs!

My interest in action-research was growing stronger and stronger. One of my last concerns regarding its use – the "publishability factor" – was slowly fading away. Suddenly, many papers giving accounts of its use in the information systems field had started to emerge [Avison et al., 1999; Lau, 1999; Lee, 1999]. But the celebratory culmination of the process was when I saw this written down: "[…] This leads us to conclude that action-research is one of the few valid research approaches that researchers can legitimately employ to study the effects of specific alterations in systems development methodologies. It is both relevant and rigorous." [Baskerville, 1999;

Baskerville and Wood-Harper, 1996]. Bingo! A quick email to Baskerville dissipated the intriguing use of the expression "[…] one of the few […]". If there were other equally suitable approaches, then I was very interested in knowing about them. As it turned out, this had been an issue of debate between the two authors, and the final formulation was chosen not to leave out things such as action-learning and the likes. That was enough for me. After all, the distinction between action-research and action-learning is so subtle that some authors even wonder if it is worth preserving.

My research log book:

- *I was right. Traditional information systems design methodologies were becoming obsolete! Other authors are claiming the same.*

  PHILOSOPHICAL ISSUES

- *Bingo again! The choice of research approach was also dead on.*

- *Must go out to celebrate.*

It was time to get busy in the field again. The technology transfer institute called me to help an important metallic cookware company rethink its information system, bearing in mind the coming millennium bug and the transition to the new European currency. Their core system was seriously dated. Y2K problems were prone to happen in both the hardware and the software. The application alone would require extensive (and expensive) reverse engineering and patching. The hardware would also need serious examination which added to the downside of being proprietary, considerably obsolete, and about to loose support from the manufacturer. Finally, the future plans that the administration had in mind called for a far more flexible information system. So, the decision was made to grasp the opportunity and rethink it top to bottom. Several business processes, such as accounting, order management, and production scheduling, were immediately identified as critical, thus requiring a reliable, tried and tested, and promptly available software solution. Enterprise Resource Planning (ERP) packages clearly addressed those needs, so a consulting company with extensive experience in the field was hired to prepare a Request for Proposal and its terms of reference. By now, it came as no surprise that their document used only plain text. No object diagrams, no detailed function specifications, and no data models. It had been written in a "language" capable of being understood by those whose contribution had been essential to the very identification of the needs: the end-users and managers. Moreover, I was now becoming aware that detailed functional specifications and relational data models were useless when we were to buy ready-made packages.

There it was again: the gap between the deliverables of traditional information systems design methodologies and the concrete field approaches needed to specify and deploy what would become the heart of the information system.

Another thread in the information system renewal was initiated to identify how sensor data from the most recent production machines could be collected for quality control. Once again, the software market held the solution, in the form of a highly specialized plant-floor package that only needed some custom coding to glue it to the ERP. But, this time, there was no need for a highly formalized acquisition process, and a much lighter analysis was carried out. Although there were great plans for this application, the consequences of its failure wouldn't be anywhere as dramatic as in the case of the ERP. As time progressed, this balanced effort was used to purchase various other packages to satisfy diverse other needs of the company.

It was interesting to see how the various applications fit in one of the four business relevance quadrants of McFarlan's strategic grid, and how that influenced the effort (and money) put into their sourcing. This distinction was seldom done in traditional information system design

methodologies since they are strongly biased towards homogeneous custom coding of monolithic solutions.

After a year of collaboration with this company, my job there was done. Their new and improved system was up and running, they were happy with it, and I had a lot to reflect upon.

My research log book:

- *The pattern keeps repeating: information systems seem to have mutated from homogeneous custom coded systems, into heterogeneous portfolios of applications.*

- *The software market is now much more diversified and mature. Most needs can be satisfied by purchasing commercial-off-the-shelf packages.*

- *That's part of the reason why traditional methodologies are failing; their deliverables are mostly directed towards custom coding.*

- *The classification of information system modules according to their business relevance is useful, not only in deciding which ones get deployed first and how they will be maintained thereafter, but also in the formality of the sourcing process.*

- *Methodologies for the present day must be more holistic and less like software engineering techniques.*

- *Being holistic, methodologies must be understandable by end-users and managers. Natural language seems a good option …in spite of its apparent lack of formalism…*

The long project in the cookware company let me stabilize several ideas I had been brewing for some time. I had now come up with the first draft of an information systems design methodology, and I was looking for a new project where I could test it. I was very confident when I outlined my proposal and gave it to my advisor to read, but became a bit disappointed when he didn't share my enthusiasm. He seemed to be partially convinced, but, for some reason, he wouldn't commit until he saw my vision applied to a real case. For several weeks I had no real project, until the head of the technology transfer institute told me about the case of a higher education school that wanted to upgrade its information system. "Here is the proof of concept I need", I thought, and promptly nagged him to get the job.

The school already had quite an extensive information system. Some off-the-shelf packages were in place to support common services, such as, student management, staff management and payroll. More specific needs had been addressed by outsourcing the development of custom applications. The school now wanted to tie all those bits and pieces together into a coherent intranet. This move would allow them to leverage the installed base by allowing access to it in a simple, uniform, and widespread basis. Furthermore, network enabling those applications would open up the possibility for cross-referencing data from the various autonomous software solutions. Some gaps in information system support were also meant to be addressed in the project. I was eager to test my methodology, so I began interviewing people with key roles in the specification of the information system. Their profiles first caught my attention. They were ordinary people, who taught for a living, English, music, math, philosophy. These were people that didn't have a clue as to what "relational database" meant. Some of them didn't even know what an intranet was. "Intranet or Internet?", some of them had asked. And, yet, I badly needed their help to design the system in a way that they would find really useful. For a moment, I recalled the frustration experienced by a colleague, when she tried to use entity-relationship diagrams [Chen, 1976] and UML diagrams [Fowler and Scott, 1997] to engage in a conversation

with end-users. She should have known better. After all, she had taught those subjects to engineering students, and even they had considerable difficulty in keeping up when things got knotty. We can't expect users and managers to be IS specialists like us. I had long been on my toes regarding those issues, and my readings confirmed that traditional methodologies – "from IBM's Business Systems Planning on up to information engineering" – inhibited effective dialogue with non-technicians [Davenport, 1997, p.160]. As the conversation with "my" end-users progressed well, I felt pleased I had sought inspiration for my field instruments on CRC cards [Beck and Cunningham, 1989; Cunningham, 1994; Taylor, 1995; Wilkinson, 1995], recognized for their pedagogic and conversational qualities and their usefulness in framing natural language descriptions. Prototypes of the intranet web pages also proved a winning choice. Although they were just mock-ups, they provided the users with a reassuring exemplification of how the system would work, giving them the chance to make substantiated suggestions on how displayed information should be organized, on precise data to input, and on the usability of interface itself.

My research log book:

- *Once again, a mix of different sourcing strategies – acquisition, custom coding – was used for different parts of the information system. By now, even these choices become facilitated by the use of McFarlan's grid: High Potential systems are prototyped, to quickly try the new ideas; for the Strategic quadrant, rapid application development is frequently the best choice, since business windows of opportunity are more important than technological excellence; Key Operational applications are generally acquired using a formal process, to identify a suitable, reliable, solution. Simplified purchase is used for Support modules.*

- *Natural language descriptions and prototypes reinforce each other's illustrative capabilities. They work very well together as instruments of discussion with users and managers about the shape of the emerging information system.*

V. FROM CASE TO THESIS

The design of this last information system went quite smoothly, surprisingly smoothly. I was prepared to make far more adjustments to my methodology in this first run. As it turned out, the most significant one was to trim down on issues regarding workflow. I sat back and recalled my steps over the last few years. My immersion in reality had really paid. Real problems, needing immediate answers, forced me to imagine feasible solutions that were immediately put to the test. Partial knowledge from various projects progressively gained form into a complete proposal that I had just tested, in full, for the first time.

I couldn't rest on my laurels, though! My meditation suddenly got drawn to my most recent readings on the generalization of results. Baskerville and Lee pointed the finger to a fallacy rooted deep in a lot of researcher's minds – that the greater the number of cases, the more valid the conclusions [Baskerville and Lee, 1999]. Natural as such reasoning may seem, the truth is that a large number of matching cases proves nothing per se. I remembered having read that our familiarity with, and almost blind acceptance of, induction could be traced back to Aristotelian times, when the evidence of multiple matching observations was used to devise and uphold general theories, to free science from tyranny and dogma [Deutsch, 1997]. However, the very principle that inductive inference would lead to valid theories was not, itself, empirically warrantable. In fact, any attempt to provide an empiric justification supporting that assertion would have to, ultimately, resort to the inductive principle itself, thus embarking on an infinite recursive

reasoning [Baskerville and Lee, 1999]. But, if trying multiple cases was not the way to claim the generality of a theory, how else could it be done?

By now, my group's interest in research approaches had matured, leading us deep into philosophy to look for answers. Karl Popper's Critical Rationalism seemed to provide a satisfactory way of handling generalization. According to Popper, scientific progress does not stem from an accumulation of observations, but from the rejection of less satisfying theories and their replacement by better ones [Popper, 1982]. Thus, a theory could be considered scientifically valid for as long as it had not been disproved, provided that the research process tried systematically to disprove it. Action-research's cyclic nature was an important contribution to achieve that. The choice of additional cases should not, however, be indiscriminately or randomly selected, but chosen on the basis of their potential to defy the generality of the emerging theory from one action-research cycle to the next. Experiences in scenarios in which the applicability of the theory was questioned, maybe due to somewhat different conditions, would then allow the extension or preservation of its generality, depending on the resulting corroboration or falsification. These considerations became an essential factor from then on, namely when selecting the last two steps of the research I felt would consolidate my Ph.D. work: cooperating in the design of the information system for a new public hospital, and restructuring the information system of the technology transfer institute itself, following the growing interests of its president in, as she put it at the time, "eating our own dog food"!

The health care project essentially corroborated the findings I had made so far, so I could not claim an outstanding progress there, apart from the pleasing comfort of watching the methodology do well on another case. Much of the now familiar pattern repeated once more: most needs were satisfied through the acquisition of standard packages. Some integration among them was needed. Several priorities and, thus, sourcing policies, were at stake. There were non-specialists to talk to, doctors, nurses and lab and administrative personnel.

The technology transfer institute, however, revealed some opportunities to improve the methodology, not so much on the underlying theory itself, but on the maturity of the process of its use. The experience suggested adjustments to make it more robust, namely to account for the possibility of team members leaving during a project, which happened in that case. Additional documents were either introduced or fine-tuned. Finally, the case provided the opportunity to test the methodology in environments not based on strong hierarchies, but, instead, organized as networks of cooperating parties.

In the meantime, the acceptance of our publications at the Americas Conference on Information Systems (AMCIS) and at IFIP WG 8.2 provided the peer recognition of the merits of the methodology [Cunha and Figueiredo, 2000; Cunha and Figueiredo, 2001]. The thesis followed suit [Cunha, 2001].

Finally the time had come. There I was, sitting in that intimidating room, ready to start defending my Ph.D. thesis before the jury and under the scrutiny of a crowd of students, colleagues, family and a number of illustrious anonymous. Right there, in front of me, on the examiner's bench, whispering to his colleagues and juggling a copy of my thesis back and forth, was none other than the guy who, back in 1997, had given my advisor's Ph.D. student a hard time with the questions regarding the validity of his research approach. Round one was about to begin.

VI. IS IT OVER?

A weird swerve of feelings, mixtures of peace and rejoice, invaded me as I realized that the public discussion had finally ended and the examiners were congratulating me on the top mark. This meant not only the successful culmination of a little over three years of hard work, but also that I had met a challenge I took personally: to be able to go seamlessly through all the questions on rigor and validity that the examiner who got my colleague into trouble years ago could throw at

me. As it happens, my action-research thesis also hinted some future avenues to look at. Soon after the discussion, three students came to talk with me on the possibility of exploring those leads. I was now passing the torch. In fact, only arbitrarily the action-research process could be stopped at the end of my Ph.D. The field of information systems is constantly changing, so it is natural that an endeavor such as conceiving a systems design methodology requires constant attention. These young students were quite confident in taking on this challenge, if still somewhat cowed by the vivid exchange of arguments at various points of my Ph.D. discussion.

VII. A PHILOSOPHICAL HANGOVER

During this process, a recurring concern had been assaulting me. A very deep concern, that went far beyond the anxiety of producing a successful thesis a sense of danger, of having stepped into the wrong train and of being unable to step down. Quite surprisingly, I was feeling increasingly comfortable in that train. Wasn't this a strong enough reason for alarm? Fortunately, the formulation of my concern had been slowly taking shape. So, I could at least know what questions I should be asking to myself: the questions of method. In a way, it all boiled down to the old issue of rigor and relevance. How could I be sure that my research was both rigorous and relevant? Could I find out any criteria to weigh that up? Would I be able to show that my research had been fulfilling those criteria?

The issue of relevance was a most intriguing one. In the old days, I had been told that it related to writing research papers capable of pleasing the readers in the corporate world. Now, this sounded even vaguer than before. Some "objective" colleagues of mine, brought up in the rationality of software engineering, had told me at the time, that it was much simpler than that – just a question of fulfilling requirements – "If you want relevance, let them say what they want and then give it to them." "What requirements?" I would ask now. My requirements seemed to have been clarifying and blurring over time, in a spiral that seemed to be converging toward some still impermanent destination. Fortunately, I had understood by now that the big difficulty of developing information systems, as well as that of developing methodologies, or of doing this kind of research, was not necessarily in problem solving, but also in problem setting [Schön, 1984]. Also, I had understood that problem setting was a long journey of trying out tentative solutions, carefully observing their effects, trying out more focused solutions, carefully observing effects, and so on, time and time again, in a "dialogue with the situation" [Schön, 1984], until some satisfying problem-and-solution pair was reached.

Now, all this seemed to have a lot to do with relevance. Relevance was not out there, waiting for us to satisfy its supposedly immutable principles. Relevance was something much more complex and blurry, something to be built as we went on. Something, I was beginning to realize that deeply touched the ethical stance of the researcher. Relevance was a negotiated process. It involved all the stakeholders of an action-research project. So, if I wanted to fulfill the still unwritten criteria of relevance, I would need to satisfy, not just the aims of my research, but also, and above all, the evolving aims of all the partners in my adventure. Bearing in mind the variety of clients I had had from the beginning of my Ph.D. work, I was glad that, honoring my engineering upbringing, I had always put so much effort into making sure that the solutions I was devising together with them, and for them, were really the ones they were looking for.

It was starting to make a lot of sense. Indeed, I had accepted projects in problematic situations that both satisfied the aims of my research and were of interest to a wide community of academics and professionals. I had established successful solutions that were recognized by all the parts involved. This clarification of real problems and construction of real solutions, experienced by real clients, in real organizations, did increase the relevance for real readers.

I was now left, at least for the time being, with rigor. This was, perhaps, easier, because I had been so concerned with it all the time. My earlier reflections on the generalization of results, inspired by the paper by Baskerville and Lee [Baskerville and Lee, 1999] and by the critical

rationalist views of Karl Popper [Popper, 1982] had gone a long way towards giving me a Popperian methodological certainty. I was now deeply convinced that the cyclic nature of action-research actually made it particularly amenable to a vision of scientific value that rested upon the systematic attempt to disprove validity of the emerging theory. In this sense, the key criteria to validate the results of action-research would be to assess the extent to which each new cycle had systematically attempted to disprove the certainties acquired in the previous ones. This would include, of course, how client settings for new cycles were chosen, bearing in mind that the major concern would not be that of having yet another cycle, but that of having a cycle that increased the potential to defy the generality of the emerging theory.

I was relieved to notice that this had been a major concern in the choice of cases for my most recent action-research cycles. True enough; the selection of the projects for the earlier cycles of my research did not conform to any special criteria. In this sense, they had not fulfilled my, now discovered, criteria for validity. This was not particularly critical, though, since the first cycles had served essentially to refine my originally fuzzy research question. It seemed wise to believe, thus, that in the earlier stages of an action-research project it is quite acceptable to allow more freedom, giving opportunity to divergent thinking and serendipity [Figueiredo and Campos, 2001], as in any creative process. It is only when the theory is gaining form that the concern with validity must tighten up and the criteria of disproval must start to be more systematically enforced. Also, as the final, more demanding, cycles put at stake the acquisitions of all the earlier ones, most of the weaknesses that might have sneaked into the emerging theory during the first cycles will tend to be easily detectable at this stage.

I was now feeling quite confident that my criteria to assess validity were as good as they could be – until the next opportunity for disproval – and that my research was fulfilling them.

VIII. WRAPPING UP

What could I conclude from this period of research in information systems using action-research? I would say that it matched the needs of a field that holds such a very close relationship with reflective practice, a field that lends itself so smoothly to convergent cycles of trial and error, toward solutions to problems that are, themselves, formulated as they are solved. This is, maybe, why rigor and relevance look so close when action-research is used. To be relevant, our work tends be very deeply cast into the needs of practice. But to be cast into the needs of practice it needs to be in tune, as much as possible, with the reflective, cyclical, nature of that practice. If rigor can be attained by systematically trying to disprove, in successive cycles, the apparent certainties acquired in previous cycles, than rigor and relevance will turn out to be, indeed, two facets of the very same reality.

This had been the experience of my incursion into action-research. I could now feel the sort of tired happiness of those arriving home from a long journey. I had traveled to the world of action-research and returned wiser and richer. Quite surprisingly, though, I had the strange sensation of having been at home all the time. Memories of the lectures my advisor gave to his informatics engineering undergraduates on "Society, Profession and Ethics", which I had enjoyed, attending during my M.Sc., were beginning to make a lot of sense to my research. I could now remember very clearly his metaphoric explanation of the distinction between a scientist and an engineer, quoted freely from Theodore von Kármán, "A scientist discovers what exists; an engineer creates what never existed". There it was, in a nutshell, part of the distinction between traditional scientific research and the world of practice, of the technical professions I was now facing in my research. The traditional scientist took mostly an analytical stance, usually concentrated on discovering the laws of nature, of the world "out there". The technical professional, on the contrary, took a projective perspective, much more concerned with the creation of a new world, a world of human artifacts. Those were the artifacts I was helping to develop. As a result, my research should try very hard to get in tune with that particular projective nature. I would certainly need to go back to

the texts on the epistemology of practice that my advisor was so keen about. That seemed to be, now, beyond doubt, my inevitable next step.

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EDITOR'S NOTE: The following reference list contains the address of World Wide Web pages. Readers, who have the ability to access the Web directly from their computer or are reading the paper on the Web, can gain direct access to these references. Readers are warned, however, that

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APPENDIX

The core challenge of the thesis was to clarify as to why existing information systems design methodologies were becoming inadequate and attempt to devise alternatives. As time unfolded, this turned into the recognition of a group of new key requirements that must be fulfilled in the present reality of IS design:

- The need to encompass legacy. The tendency to abandon old systems and replace them with entirely new ones was being challenged.   The fact that the innovation of today is the legacy of tomorrow and that the increasing costs of converting old data, overcoming resistance to change, retraining staff and avoiding productivity drops recommended that existing systems should be utilized as much as possible.

- The need to acknowledge and take advantage of the full range of systems procurement alternatives, such as outsourcing, application service provision, traditional in-house development and, in particular, the acquisition of common off-the-shelf (COTS) solutions provided by a software market that was now capable of satisfying most business requirements.

- The need to acknowledge that, as a consequence of the previous two trends, organizational information systems had mutated into portfolios of heterogeneous solutions, of different ages, supplied by different vendors, at dissimilar stages of technological evolution.

- The need to acknowledge the different business values of various solutions in the application portfolio and their implications on resource allocation for procurement and maintenance throughout their lifecycles.

- The need to incorporate the design of the intranet and extranet, whose dynamic web pages were becoming gateways to the underlying heterogeneous applications making up the portfolio, thus leveraging them to new uses and aligning them with the business needs.

- The need to do all this is a way that supported dialog and was understood by non-technical end-users whose business knowledge was fundamental in determining how to accomplish the best design.

- Finally, the need for simplicity, to cope with increasingly fast business pace and minimize overhead for users and managers whose contribution was essential.

The result was the proposal of an Evolutionary Information Systems Design methodology (EISD) enabling shared construction, by mixed non-technical and technical teams, of high-level specifications capable of meeting such requirements.

The approach was centered on two key concepts: the "business entities" and the "responsibilities" for which those entities were accountable. "Business entities" could represent several realities, ranging from clearly defined "departments", typical of more mechanist organizations, to versatile configurations, or functions, or teams, or even individuals, typically emphasized in more organic companies. The "responsibilities" were the major "services" that business entities provided to their environment. To use such services, client entities must follow predetermined interaction protocols. For a simple illustration of the use of the two concepts, we can think of the human resources division of an enterprise as being a business entity and the services it provides, such as "authorize vacations," as responsibilities. The inner workings of a responsibility may be

supported by full-blown applications or ERP modules, such as stocks, financials, or employee relationship management. By systematically identifying and documenting all business entities and their interactions, via responsibilities, this modeling approach afforded an organizational model that blended in the information system.

To ensure field instruments were accessible to executives and end-users alike, inspiration was sought on CRC cards [Beck and Cunningham, 1989; Cunningham, 1994; Taylor, 1995; Wilkinson, 1995], recognized for their pedagogic and conversational qualities and for their usefulness in framing natural language descriptions. Business entity cards were used to document each business entity: name, a list of its responsibilities, and its client entities. Responsibility cards were used to detail information about each such service: name, purpose, importance, main rules, exchanged data, business procedure to carry it out, and available information systems support (e.g. applications in use). For the last two items, both current situation and intended changes were identified in order to facilitate the definition of alternative evolution roadmaps for different scenarios. In addition, intranet and extranet prototypes were drafted, to further clarify the interaction between the responsibility and its environment. Responsibilities for which no information systems support existed were also documented, as their inclusion in the model afforded better understanding of the overall functioning of the organization, bearing in mind that future IS support could be considered when deemed convenient.

Since IS support is defined in terms of required functionality, not a particular technical solution, the full range of procurement alternatives is available. The description can be used as the basis for a request for proposals for acquisition, for application service provision or for outsourcing. The latter, together with in-house development, can use the document as the business case and preliminary requirements for a software engineering process. In fact, the choice of sourcing alternative is guided by the relevance of each information system module, in line with McFarlan's grid classification [McFarlan, 1984; Edwards, et al., 1995; Ward and Griffiths, 1996]: high potential applications are generally prototyped in-house; strategic quadrant modules are frequently coded using rapid development tools, either in-house or through outsourcing; key operational solutions are selected from mature alternatives available in the software market; and support applications are purchased following a low overhead approach, with emphasis on low cost, standardization and longevity.

Since information system modules become encapsulated by the responsibilities and are accessed using the defined interface protocols, instantiated by the intranet and the extranet, heterogeneity is naturally encompassed. The age, the technology, or the vendor cease to be an issue, as long as the solution keeps performing according to what is expected. Furthermore, evolution becomes almost seamless, as modules can be replaced without disturbing the environment as long as responsibility interfaces are preserved.

The main deliverable of the Evolutionary Information Systems Design (EISD) methodology is a report made up of the various entity and responsibility cards and a CD or DVD with intranet and extranet prototypes. This document provides a holistic, yet manageable, view of the whole organization – the way it works, and planned changes – both in terms of business procedures and information systems support. Open discussions about possible evolutions can take place on a permanent basis by simply picking up the relevant cards, sketching new views and reflecting upon them, making the design process a light, shared, and continuous endeavor.

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